IS EFFECTIVE EVIDENCE-BASED MATHEMATICS TEACHING POSSIBLE?

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Four key questions face those involved in trying to develop evidence-based mathematics teaching: (i) What counts as a good question for a review?; (ii) What counts as good evidence concerning which classroom practices work best?; (iii) How can such evidence be transmitted to teachers?; and (iv) How can teachers be encouraged/directed to adopt more effective classroom practices? Current initiatives in this area suggest that the value of trying to develop evidence-based education may lie in stimulating teachers, teacher educators, researchers and policy-makers to consider in an intelligent fashion what research can tell us about good practice, but this enterprise is unlikely to deliver an unequivocal blueprint for effective classroom practice.

INTRODUCTION

Over the last twenty years, there has been a marked move in the United States towards the adoption of 'evidence-based education'. In essence, this refers to the idea of evaluating the best available research evidence concerning which classroom practices are most effective in delivering the learning outcomes being sought. This model, which has been widely applied within medicine, where treatments have been evaluated through the use of randomised control trials (RCTs), has now been increasingly used by education policy makers. In the United States this has given rise to the establishment of web-sites where reports synthesising the results of largely RCT-based studies dealing with particular classroom practices can be found.

Within the United Kingdom, the work of the Evidence for Policy and Practice Information and Coordinating Centre (EPPI Centre) at the University of London Institute of Education, reflects many of the same objectives and assumptions developed in the United States and has spear-headed an initiative in this area through its promotion and oversight of such 'systematic reviews' commissioned by the DfES, now DCSF.

In 2003 a Mathematics Education Review Group was established, co-ordinated by Chris Kyriacou and Maria Goulding at the University of York, to undertake systematic reviews on aspects of the teaching and learning of mathematics in schools. At the last count, 38 such review groups have been established under the aegis of the EPPI Centre (http://eppi.ioe.ac.uk). Each such review group largely comprises teacher educators and educational researchers drawn from several universities, together with some members representing school teachers, policy-makers, LEA-officials and research students. In line with EPPI Centre expectations, several members of the Mathematics Education Review Group have attended training sessions at the EPPI Centre regarding how to follow the Centre's procedures for

conducting a systematic review, including how to operate the software the Centre has developed for reviews.

To date, the Mathematics Education Review Group has carried out four reviews (Goulding and Kyriacou, 2007; Kyriacou and Goulding, 2004, 2005; Kyriacou and Issitt, 2007). This is now a good time to take stock of what we think have been the benefits and shortcomings of such work, and to address whether the main purpose of this enterprise, namely, to help establish effective evidence-based mathematics teaching in schools, is sustainable and well-founded.

KEY QUESTIONS

Four key questions will be addressed in this paper: (i) What counts as a good question for a review?; (ii) What counts as good evidence concerning which classroom practices work best?; (iii) How can such evidence be transmitted to teachers?; and (iv) How can teachers be encouraged/directed to adopt more effective classroom practices?

(i) What counts as a good question for a review?

Each systematic review starts by establishing a question to be addressed. This tends to be as narrow, specific and precise as possible. For example, to ask a question such as "What is the most effective way to teach geometry?" would be regarded as far too wide in its scope. Such a question would cover a broad age range, would cover a whole range of teaching approaches, and would deal with all aspects of geometry. This would make the review far too unwieldy. Much more appropriate would be to focus on a particular aspect of this, such as perhaps, "Can small group discussion enhance year 5 pupils' understanding of the properties of different types of triangles?". However, if the question is too specific, there is a danger that a search for research evidence based on studies which have evaluated classroom practice in these specific terms may not be fruitful.

Moreover, if every systematic review focussed on very specific questions, the number of separate reviews that would be needed to build up an evidence base concerning all aspects of the teaching and learning of mathematics would become voluminous. As such, the framing of a question for a systematic review needs to be focused enough to be manageable and meaningful, but not so focused that its value in building up an evidence base will be too limited.

Another major issue concerning the framing of a review question, is that the question is likely to be value-laden, in the sense that questions are devised or commissioned by those who have already framed a context in which this question is valued. We need to ask why this particular question has been privileged over other questions that could be asked. As such, asking a good question of itself begs a number of issues.

It is thus clear that epistomologoical constraints attendant upon the demand for evidence limits the utility and applicability of a review to those questions which are

answerable in terms of the availability of relevant studies and which are framed within the particular discourse or terms of reference of those commissioning the review.

(ii) What counts as good evidence concerning which classroom practices work best?

Perhaps the greatest challenge facing evidence-based education is being able to identify 'good evidence' for a systematic review. There are two major problems with identifying good evidence. First, most studies that exist in the research literature have been designed with other research questions in mind, not the review question. As such, the review needs to be able to identify those data in a study which throw light on the review question, even though the way the study was designed and the way the data are presented in the study, may not be entirely helpful or appropriate from the perspective of the review question. For example, if we take the question "Can small group discussion enhance year 5 pupils' understanding of the properties of different types of triangles?", the study may have included data on pupils from years 3 to 6, without presenting the data for year 5 separately; or the study may have presented data in which the learning outcomes related to triangles were subsumed within broader outcome data.

Second, the study included the review may throw light on just one aspect of the review question. For example, if we take the question "Can small group discussion enhance year 5 pupils' understanding of the properties of different types of triangles?", we may come across a study that looked at how teachers and pupils felt about engaging in such lessons, without presenting hard evidence (i.e. RCT-based evidence) that the activities were more effective than other activities in delivering these precise learning outcomes.

As noted earlier, systematic reviews tend to privilege RCTs as providing the strongest quality of evidence concerning the effectiveness of particular classroom practices. However, many studies in mathematics education tend to focus on issues concerning teachers' attempts to understand and develop their own practices, in which qualitative data are used to provide insights into the context and processes involved. Whilst qualitative data may not present 'hard evidence' of the effectiveness of the classroom practices being investigated, they often do provide a basis for a better understanding of the factors and issues influencing its effective use. These are as valuable, and sometimes more valuable, to the review, than studies presenting hard evidence. In this sense, the privileging of hard evidence will limit the utility of the review.

Moreover, when looking at classroom practice, the variation in the 'treatment delivered' can be enormous. Several studies, for example, may all have employed small group work, for example, but the way such work was organised, may vary markedly from study to study (indeed in some studies, treatment variability may be evident across the different teachers in the same study). Thus, to what extent, all the pupils in these studies can be said to have experienced the same treatment, is itself very suspect.

The title of this paper includes the word 'effective'. This is to denote that whilst it may be possible to encourage 'evidence-based practice', if the evidence is faulty, then such practice will not deliver more effective teaching. As such, the whole thrust of evidence-based education hangs or falls on how secure we are that the research evidence justifies the use of particular classroom practices.

(iii) How can such evidence be transmitted to teachers?

Despite the issues covered above, let us assume that the studies in a particular review offer very strong evidence that a particular classroom practice (e.g. small group work) is much more effective in producing particular learning outcomes (e.g. understanding the properties of triangles), in this particular context (e.g. typical year 5 pupils) compared with other classroom practices. How can such evidence be transmitted to teachers?

The DCSF and the EPPI Centre, as well as many other agencies, are well aware of the immense number of studies that are published each year making claims about effective classroom practices. Can these be drawn to the attention of teachers in a meaningful way? Ideas such as having websites with digests of such reports, or creating journals/magazines that can be sent to schools with such digests, all face the issue that teachers rarely have the time to consider such information.

Digests are likely themselves to present over-simplified summaries of what a review has to say, and will almost certainly gloss over any important caveats. When teachers, should they be able to, consult a fuller length account of the review findings, will this be written in a way that teachers can readily make sense of and apply to the own circumstances? For example, the evidence about the effectiveness of small group work may be based on studies done with experimental classes where small groups where removed from their normal class. Would such a practice be viable in the normal circumstances in which schools operate? Would small group work activity still be effective where normal class was divided in small groups, rather that where one small group was extracted from the class one at a time?

(iv) How can teachers be encouraged/directed to adopt more effective classroom practices?

Again, let us suppose that the classroom practice can be drawn to the attention of teachers in a way that they can understand and apply to their circumstances. How can teachers be encouraged/directed to adopt these practices? For most teachers most of the time, their current practice tends to be maintained through inertia. Current practice is familiar, well rehearsed, and problems concerning its effective use are well known and can be anticipated. As such, most current practice can be delivered easily and skilfully. Adopting a new practice involves risk and uncertainty.

The widespread advocacy of a new practice by incorporation, for example, within a national strategy, in which teachers' use of the new practice can be evaluated through inspection and appraisal mechanisms, can still be subject to modification and

resistance. For example, if teachers find that the practice is not easy to implement or does not readily deliver the outcomes claimed, the practice will soon be offered only in a cursory manner or in a way not intended by its advocates.

CONCLUSION

Can we imagine a future world, in say 20 years time, in which we can say that the teachers' teaching is evidence-based, in the way being envisaged by advocates of evidence-based education? We think not. First, the nature of evidence remains far too inconsistent and equivocal. Second, all classroom practice is context-specific and value-laden. Teachers will continue to adjust their practice so that it works effectively in the range of particular context they have to operate within, and which at the same time adheres to their values concerning the nature of teaching and learning. Teaching is not a mechanistic activity in which all teachers can gradually come to teach in almost identical ways across a range of contexts. The value of trying to develop evidence-based education may lie in stimulating teachers, teacher educators, researchers and policy-makers to consider in an intelligent fashion what research can tell us about good practice. This enterprise, however, can not provide a blueprint for an Educational Utopia.

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